

the pulse is full and strong, and is decidedly *above par*, the lancet should be used promptly and boldly, to guard against the production of local lesions; but we should at the same time bear in mind that this stage is of short duration; that we are dealing with a specific disease which has a strong tendency to early collapse, and that we may soon have "need for all the strength we take away." These are cardinal points, which I take to be well settled in the minds of experienced and judicious practitioners.

In conclusion, I beg leave to remind the reader, that I am writing of the yellow fever of Mobile, as we have seen it during the epidemics and endemics of 1837—'39—'42—'43 and '44, which have afforded an ample field for observation. He who is ignorant of the various types in which this Protean disease appears in different years, and in different latitudes, must either not have read, or read to little purpose the history of yellow fever. In one epidemic, we are told the lancet is the sheet anchor—in another, it is death. This difference occurs to a limited extent in Mobile, but the *rule* is, beware of the lancet.

Even the blood-thirsty Rush, (whose philanthropy we all admire,) after singing so loudly the praises of the "*coup sur coup*" system in 1793, and pointing triumphantly to cases where he had drawn 100—150 and even 200 ounces of blood, is forced by stern facts to lower his tone in '98, and in 1805 he tells us, that many of his cases would bear but one bleeding, and others none at all, and this is the point to which experience and sober judgment must inevitably come.

MOBILE, November 10th, 1844.

ART. II.—*Strictures on the use of the term Congestive as applied to low forms of Fever, with some general observations on the pathology of these diseases.* By ISAAC PARRISH, M. D. (Read before the Philadelphia Medical Society, at the session, 1844—45.)

THE term *congestive* is now extensively employed to designate certain alarming, and oftentimes rapidly fatal symptoms which arise in the course of malarious fevers, more especially as they prevail in the southern and southwestern portions of the United States.

The fatality of these fevers, in certain seasons and localities, carries terror to the inhabitants, and the name by which they are known has become established in medical nomenclature as descriptive of a malady in which congestion of the internal organs is considered the most prominent pathological state.

I propose, in the present paper, to take exceptions to the term by which
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the disease is now generally designated, on the ground that it is not expressive of its true pathology, and is calculated to lead to serious errors in practice. As the basis of these remarks, I shall adopt the description of the disease as given by two competent and faithful observers residing in different portions of the country, who have portrayed it as it occurred under their immediate observation, both in the intermittent and remittent type. Those gentlemen who listened to the paper of Dr. Charles Parry, of Indianapolis, read before this society in the winter of 1842, will recollect the thrilling interest excited by his account of several epidemics which had occurred under his notice at that place—and the interesting discussions growing out of these observations. This memoir, abbreviated and condensed, appeared in the *American Journal of Medical Sciences* for July, 1843—and is now referred to, as containing one of the most graphic descriptions of the disease termed *congestive fever*, in its intermittent form, which has yet been published.

The chief points in the paper of Dr. Parry, may be thus summarily stated. The *congestive fever*, in the vicinity of Indianapolis, prevails at the close of summer, and during the autumn until the appearance of heavy frost, occurring simultaneously with the ordinary remittent and intermittent fevers of that region, with this difference, that the former are observed along the low marshy grounds skirting the rivers, while the latter usually occupies the table-lands. The disease in this locality generally assumes the intermittent form, the first paroxysm differing but little, to an unpracticed eye, from an ordinary seizure of intermittent fever, but to one accustomed to the disease, presenting certain physiognomical traits indicative of its true character. After the first seizure, and during the intermission, the patient moves about as usual, feeling, however, remarkably languid, and at the end of twenty-four or forty-eight hours, according to the type it may assume, he is attacked with a second paroxysm. This is marked by extraordinary violence, generally terminating in from three to six hours in reaction, and another intermission, or in death. The symptoms which mark this period of extreme illness, and which are supposed to characterize the disease as *congestive*, are peculiar. There is a sense of universal rigor, and extreme coldness, intense and insatiable thirst, the patient constantly imploring the attendants for cold drinks, and begging to be placed in cold water, as the only means of relief.

The bowels are disturbed—bloody, serous discharges pour out copiously, without pain or effort, but a short period intervening between the dejections. In some instances pure blood is discharged, and Dr. Parry recites one case in which as much as twelve ounces escaped at one time. The general appearance of the evacuations is that of bloody serum, without smell, and comparable to the "water in which recently killed beef has been washed." There is but little abdominal pain or tenderness on pressure; sometimes vomiting, with great irritability of the stomach, and a sense of weight and burning at the epigastrium. The respiration is irregular, sighing and peculiar,

with a horrible sense of suffocation, which induces the patient to insist on getting up, to sit by an open window, that he may catch every breath of air. There is no cough or mucous rale. There are great restlessness and jactitation, and oftentimes surprising muscular strength until within an hour or two of dissolution—the sufferer being able to jump out of bed and walk about while he is pulseless. The surface is cold, livid, and covered with a sticky, clammy moisture, and the skin of the hands is wilted and shriveled, as in the collapse of cholera maligna. The pulse, when it exists, is small, thready, and very frequent, sometimes more developed, but irregular and intermittent. The capillary circulation is torpid. The intellect is generally unclouded, even while the other symptoms are so terrific—there is no tinnitus aurium, blindness or perverted vision—there are some cases, however, in which delirium occurs, and in some the patients are comatose. The discharges of urine are scanty or suspended altogether.

The above assemblage of symptoms indicate a highly malignant and mortal disease, of an intermittent type, occurring in a miasmatic district, at a season favourable for the development of this poison, and ceasing on the occurrence of frost. The description here detailed will be found quite analogous to that given by authors, of the malignant or pernicious intermittent of warm climates—sporadic cases of which are occasionally seen in this latitude, or occur in persons just arrived from the south. It is now known as *congestive fever*.

The only difference between this form of the disease, and that described by other observers, is, I think, in the type, and perhaps in the degree of violence. In the other varieties, the disease comes on more gradually, with that feeling of malaise which precedes the ordinary remittent fever; the symptoms of remittent being afterward developed, and suddenly assuming, at the end of four or five days perhaps, an alarming aspect, which determines its true character, and gives it the title of *congestive*.

An account of the remittent form of the disease as it prevails in Mississippi and Louisiana, was published in the *American Journal of Medical Sciences*, for April, 1844, by Dr. R. G. Wharton of Grand Gulf, and may be considered as a fair description of the disease, known as *congestive fever* in those states. In this memoir, the same general phenomena, described by Dr. Parry, may be recognized. The cool, clammy skin, quick, thready pulse, sighing respiration, sense of suffocation, intense thirst, precordial heat, serous discharges from the bowels, jactitation and restlessness, unusual muscular strength, &c. &c. The rapidly fatal termination of the disease, when once fairly formed, and the necessity of commencing the treatment in anticipation of its approach, as the only means of saving the patient, are alike insisted upon by both writers.

Assuming the above summary to present an outline of the symptoms which distinguish this form of fever, as a distinct and peculiar disease,

let us now inquire into its pathology, and briefly examine the grounds upon which the term *congestive* is applied to it.

This inquiry involves considerations of a very important character. It is too often the case, that men act from the influence of names—and under the cover of a false term, commit serious errors, and form wrong conclusions.

With certain terms, particular ideas are associated, and the mass run away with the name, without investigating its true meaning, or entering into any examination of the phenomena or ideas which it purports to represent.

In medicine, this is a great evil; it leads to looseness and vagueness in pathology, and to varieties of opinion and practice based upon different views in regard to the meaning or intent of certain terms, which may represent different, and even entirely opposite states of the system.

To a certain extent this is inevitable, arising from the imperfection of language, which must always exist, and which is common to all branches of knowledge; but still it must be confessed that many defects exist in medical terminology which are remediable, and which it is our duty to expose and correct. In a science where words influence practice, and thus indirectly affect human life, it is particularly important to use terms having a definite meaning, and to beware of the adoption of those which may lead the inexperienced into dangerous errors.

The origin of the term *congestive*, as here applied, can doubtless be traced to the peculiar views of a certain class of physicians in reference to the pathology of the disease in question, while its continuance is now urged upon similar grounds, by a large number of intelligent practitioners. The word is derived from the Latin *congero*, to amass, and, in a medical sense, conveys the idea of a turgescence or fulness of blood in a particular part. Used in the present case, it should indicate that the chief characteristic of this variety of fever, consists in the overloaded or turgid state of a particular organ or organs, that the phenomena peculiar to it are produced by this fulness; that this is the *fons et origo* of the alarming symptoms which mark it, and the cause of the speedy death of the patient, and hence point out as the primary indication in its cure, the unloading of the oppressed organ or organs, by means of general or topical depletion. A necessary connection appears to exist in the minds of physicians between the term *congestion* and the depletory or antiphlogistic practice, just as it does between *inflammation* and the use of the lancet—and in this fact lies the great danger of the name.

I am aware that there are many experienced practitioners who have seen much of this disease, and who are fully convinced of the dangerous tendency of the depletory practice, who still adhere to the term *congestive*, under an idea that there is an accumulation of blood in particular organs, by which they are oppressed, and their functions impeded; but that this turgescence is of a passive character, and results from a want of due vitality and energy in the oppressed organs. This is the doctrine of Drs. Parry and Wharton, to whose essays I have referred. Under this view of the case, *congestion*

means nothing more than the settling of blood about the central organs and great vessels, because there is not force enough in the nervous system to act upon the organs which propel it through the body. The congestion is here the consequence of diminished nervous power, and is nothing more nor less than that state which occurs in all cases of sudden prostration. It is not a pathological condition peculiar to this form of fever, or necessarily connected with it, except as one of the phenomena of nervous exhaustion, or of the dying state when this event happens.

It occurs to a certain extent in fainting, in nervous shock after severe accidents and operations, and under a great variety of circumstances where its existence is not noted as an element in the pathology of the disease. I contend, therefore, that the use of the term *congestive*, *in this sense*, is no more appropriate in the disease under consideration, than it is to any prostrate condition of the nervous system, induced from other causes. If it be, then we might say that every individual who dies of any disease or accident, is labouring under *congestion*, because, as the heart ceases its action and the tissues lose their tone, the blood becomes congested, or, more properly, settles in the heart, large vessels, lungs and other central organs, leaving the capillaries and smaller vessels by the simple force of gravity.

Restricting the term, however, to the expression of a pathological condition, in which, from some cause or other, there is a tendency to a flow of blood to some one or more internal organs, by which a state of "suffocated excitement" is induced in these organs, threatening their speedy disorganization, if not relieved—what is the argument for the existence of this state in the disease termed *congestive fever*?

It is contended by the advocates of the doctrine of *congestion*, that the essence of this disease consists in an overloaded condition of the vital organs, dependent on the violence of the exciting cause—acting, some say, primarily upon the capillaries of the skin, and others say through the nervous centres; that instead of being a disease of low action, it is one of the highest degree of activity, and the patient dies if not relieved, from the disorganization of vital organs, and not from nervous prostration. To illustrate this view of the case more perfectly, let us revive the simile employed by one of the most eloquent defenders of this doctrine, while arguing the question on this floor, during the session of 1842. The blood, said he, is driven by the tonic contraction of the capillaries, to the central parts of the body, where it is pent up in the heart, lungs and viscera, just as the water of a mill-race is confined by the shut-gates—if the gates are not opened when the water reaches the summit, there will be an overflow, and so if an outlet is not opened for the blood thus confined, there will be a fatal overflow, breaking down the tissues and destroying the structure of organs necessary to life. Hence the accumulation must be relieved, and the only outlet is through the medium of an open vein—this is the flood-gate which must be hoisted, and the sooner it is opened the better for the safety of the patient. This method

has the merit of consistency ; if the danger arise from congestion of internal organs, depletion general and local is the most rational remedy.

It is further urged that all the symptoms indicate a congested state of the vital organs. The sense of praecordial heat and oppression, with constant and unsatisfied thirst for cold drinks, and even for ice, is assumed as indicative of congestion of the mucous membrane of the stomach—the irregular and sighing respiration, so peculiar in this complaint, is adduced as evidence of a mechanical impediment to the passage of air through the lungs, from vascular turgescence of these organs—the bloody and serous dejections are supposed to denote active congestion of the intestinal mucous membrane—the absence of biliary secretion is said to indicate deep engorgement of the portal circulation and of the liver—while coma and delirium, when they exist, are attributed to congestion of the brain.

A reference to post-mortem examinations will throw but little light upon either side of the question—as these have been so rare as not to be brought into the argument ;—it is probable, however, that the advocates of the doctrine of congestion might suppose their position strengthened from this quarter, inasmuch as a certain amount of congestion of the vital organs is frequently observed as a post-mortem appearance in persons dying from all diseases, and more especially after sudden illness.

The chain of argument here presented is, I believe, a correct view, though necessarily very brief, of the views of those who hold to the doctrine of congestion, as applied to the disease under consideration. It appears plausible, and has been generally entertained, not only by those physicians who advocate depletion, but by those who rely mainly upon large doses of calomel, and by that large and increasing class who place their confidence in stimulants and tonics : the latter considering the congestion to be secondary and passive, and the others viewing it as an active pathological state.

I will now endeavour to state some of the reasons for maintaining a different opinion, and for urging the disuse of a term which I seriously believe has led to lamentable errors of practice, and which is now employed to represent two precisely opposite conditions of the system, by men who are arrayed against each other upon the most important questions of practice.

Before proceeding to this part of the subject, allow me to refer to the source to which I am indebted for many of the views now presented.

Those gentlemen who were the private pupils of my lamented father, or who attended his lectures on the Practice of Medicine, will remember the doctrines which he taught in reference to fevers of a low grade, and the points on which he differed from the prevalent opinions of other medical teachers of that period. The theory of Armstrong, which attributed the death of the patient in fever to congestion of the internal and vital organs, and that of Rush, which considered the danger, in violent cases of fever, to arise from “suffocated excitement” in the vital organs, were at that time in the ascendant. Upon them was based the depletory practice, so extensively

pursued, for the last thirty or forty years, and still urged by the admirers and advocates of these doctrines. In the epidemic typhus fever which prevailed with such fearful mortality in Philadelphia, in the winter of 1812-13, and of which Dr. Rush died, my father took decided ground against this doctrine in reference to typhus, and in opposition to the prevailing practice, pursued the stimulating and tonic plan of treatment in this disease. The results of this course were most gratifying, and contributed in no small degree to elevate him to that rank as a practitioner which he afterwards held. The facts and arguments by which he was led to this conclusion, formed the subject of several lectures which were embodied in his course on the practice of medicine. They apply with equal force to the disease now termed congestive fever, and were adduced by him, during the late years of his life, against the use of this term, as expressive of the peculiar characters of the malignant intermittent and remittent fevers of the U. States, which he believed to partake of the same general features as the malignant typhus, although differing in some important respects.

To return, then, to the question of the propriety of the term *congestive* as now popularly employed.

A review of the symptoms which characterize the fevers termed *congestive*, so far from indicating the existence of this condition, will, I think, on a more rigid analysis, furnish strong evidence against it. We remark first—the languor, lassitude and general malaise, which usher in the attack, indicative of a depression of the nervous system, are the first links in the chain of morbid actions, in *this* as in all other varieties of fever. The chill which follows and which in the first paroxysm may not be remarkably violent, is succeeded by a feeble reaction, and by an inability in the system to establish a complete solution of the paroxysm as in ordinary intermittents. This fact manifests the unusual depression of the vital power, which is fully made known on the access of the succeeding paroxysm, by the frightful phenomena which appear.

All the symptoms which now present themselves, and which have been so well described by the writers referred to, indicate extraordinary and alarming diminution of nerve power from the energy of a poison acting primarily on the nervous centres, and not, as we contend, from *congestion* of the vital organs.

I shall endeavour to sustain this view of the subject, by an individual examination of the several symptoms, attributed to *congestion*, and by a comparison of these with like appearances or phenomena, which occur in other states of the system, in which nervous exhaustion is universally admitted to be the prevailing condition. If this position can be maintained, I shall hope, at least, to have removed one great obstruction to the proper understanding of the subject, and to have accomplished something towards reconciling the theory and practice of those gentlemen who advocate the stimulating plan in the treatment of this fever.

The advocates of congestion regard the intense and insatiable thirst, which distinguishes this form of fever, as a strong evidence of internal heat and excitement in the vital organs, especially in the stomach. The desire for ice and cold drinks being, as they say, a natural craving to cool and soothe the injected and irritated organs. Now it is well known that thirst occurs under two opposite conditions of the system, viz., in a state of *excitement*, and under the influence of *prostration*—and that in the former case it is less craving and more easily satisfied than in the latter.

The thirst of the exhausted state is an incessant, uncontrollable and urgent desire for drink, which cannot be appeased by the most copious draughts. Its urgency is well depicted by Dr. Parry, in the memoir heretofore quoted, and has been remarked by all the writers on low fevers, as a distinguishing feature of these complaints. It occurred in the epidemic typhus fever of Philadelphia in 1813, and is noticed by the New England writers on the same disease. It is also one of the most distressing symptoms in malignant cholera and in yellow fever. It is akin to the thirst which occurs after uterine hemorrhages, where the patient lies cold and pulseless from *loss of blood*. The surgeon is familiar with it after severe accidents in which the nervous system has received a violent shock, and in cases where large quantities of blood have been lost in operations. It is noticed by writers on military surgery, as amongst the most distressing torments of the wounded on the field of battle.

It was a prominent complaint amongst those mortally wounded during the late riots in this city—as those who witnessed the sufferings of these unfortunate men can testify. It is a thirst peculiarly indicative of a prostrate condition of the system, and has no relation whatever to congestion of the internal organs, unless the absurd contradiction be assumed that patients labouring under the effects of enormous hemorrhages, are at the same time the subjects of an overflow of blood upon vital organs.

The respiration so peculiar to this fever, has likewise been supposed to be owing to a mechanical impediment to the passage of air through the lungs, from an undue accumulation of blood in these organs. It is represented as *irregular* and *sighing*, and to be accompanied with a distressing sense of suffocation. Now this condition of the respiratory apparatus is especially noticed by Dr. Marshall Hall, and others, to occur in prostrate conditions of the system from copious hemorrhages and other causes. It resembles the respiration which occurs in fainting, in hysteria, &c.

It is not the laboured, hurried and panting respiration, with stricture across the chest and suffusion of the countenance, which occurs in mechanical obstructions. It more closely resembles the breathing which might be supposed to accompany prolonged syncope. The sense of suffocation which coexists with it, is also found to occur in similar states of the system, where no suspicion of congestion of the lungs is entertained. One of the most striking instances of this fact occurred to me about a year since. It

was the case of a young woman who was labouring under anaemia, from a protracted uterine hemorrhage. The sense of suffocation and desire for air were distressing attendants upon her case—she died from exhaustion, and almost in the act of crying out for the hoisting of the windows and the opening of the doors. Post-mortem examination in this case revealed a perfectly healthy condition of all the organs—the lungs, instead of being engorged with blood, were unusually pale and bloodless, as were all the tissues of the body. Such facts must be familiar to most physicians.

It is somewhat surprising that the influence of the eighth pair of nerves has not been adverted to by the writers on this and other varieties of low fever, to explain the peculiar phenomena which affect the respiration. In pursuing the hypothesis of congestion, the fact seems to have been overlooked, that the functions of the lungs are liable to be impeded from a want of nervous power, as well as from an overflow of blood. Bichat performed the section of the eighth pair in animals, and found that the breathing became once very laborious, and continued so until the death of the animal.

Dupuytren repeated these experiments some time after, for the special purpose of determining the degree of influence which the brain exercises over the lungs, through the medium of the pneumogastric; and published the result of his investigations in a memoir of remarkable interest. He arrived at the conclusion that the animals upon whom this section was made, die of asphyxia. Blainville, Provençal and others, arrived at a like result, after laborious research.

Le Gallois has since carried the experiment further, by the division of the recurrent nerve, distributed to the larynx, and found that death occurred from suffocation in less time than by division of the par vagum; and further, that by opening the trachea below the larynx, the sense of suffocation was immediately relieved, and the animal was resuscitated, thus proving that the difficulty arose from a contraction in the opening of the glottis.

These experiments were made the subject of an elaborate and able report, read before the French Academy in 1811, and contained in Dr. Le Gallois's interesting work on the "Principle of Life." Their relation to the subject before us, and the exceedingly valuable facts developed by them, render them well worthy of notice in this connection. The depressed state of the circulation which is so remarkable in this form of fever, and which has been attributed to congestion of the vital organs, may also be explained by reference to these experiments. In all the animals in whom the pneumogastric was tied, the beating of the carotids became immediately feeble, and these vessels lost their tension and fulness—vomiting also occurred in all the animals capable of performing that act. Without entering at large into this branch of the subject, which would prolong this article beyond proper limits, it may be stated as a general conclusion that the disturbances in the several functions of the heart, lungs and stomach, which have been attributed to congestion in these organs by the advocates of this doctrine, may be ex-

plained upon more rational principles, and with a much greater amount of direct evidence in its favour, upon the principle of diminution of the nervous power belonging to these organs, through the medium of the pneumogastric and its recurrent branch.

There is, in fact, no little similarity between the phenomena produced by a division of the pneumogastric and the cold stage of a malignant intermittent.

A similar analogy has been noticed by Andral between these phenomena and the symptoms arising from dangerous or fatal hemorrhages; while the sagacious Dr. Marshall Hall has remarked, that a close resemblance exists between the respiration of a patient labouring under exhaustion and one supposed to be oppressed by a congestion of blood in the lungs.

Another proof of the existence of congestion, advanced by the advocates of this doctrine, is that the condition of the capillaries of the surface of the body, the general coldness and moisture which prevail during the disease, indicate a diminished supply of blood in the superficial vessels; and it is urged, that during the chill, there being a tonic contraction of the capillaries, as evinced by the rigidity and coldness of the surface, blood is forcibly thrown back upon the vital organs, thus causing their congestion. This is a mere hypothesis; and in our ignorance of the formation and functions of the capillaries, and their relation to the general circulation, it is altogether premature to assume them to possess the power of tonic contraction, much less to contend that they have the propelling energy here attributed to them.

Drs. Parry and Wharton both describe the skin as cold and clammy. The former remarks, that "the capillary circulation appears stagnated, as indicated by the lividness and coldness of the surface of the body, the palleness remaining after pressure, a cold, clammy, sticky sweat, sometimes extending from head to foot, but frequently only over the face and neck; the skin on the hands is shriveled, wilted, and they look as if they had been soaked in ley, like a washerwoman's hands." From this description, the capillaries would appear to be *enfeebled* rather than excited, into *tonic contraction*, and to partake of the general prostration which pervades the whole system. The moisture which covers the surface manifests also an enfeebled and relaxed rather than a contracted and rigid state of these vessels.

There are, in fact, that death-like coldness and clammy sweat which occur after hemorrhages, accidents, &c., and which are always indicative of imminent danger—and it appears to me just as reasonable to contend for a tonic contraction of the capillaries and internal congestion, under these circumstances, as in the case before us. To illustrate this view of the subject a little further, let us take a case from surgery. A man falls from a height on his head, and receives a severe concussion of the brain. He immediately loses his sensibility—vomits, becomes pallid and cold, and, were his sensations not obliterated, he would probably complain of a chill; his respiration is irregular, sighing, or perhaps stertorous, and his pulse beats feebly and

irregularly. Shall we say, in this case, that there is tonic contraction of the capillaries, driving the blood towards the central organs, or that having left the surface it must necessarily have become engorged in these organs, and hence that he is labouring under congestion, and must be immediately depleted to save his life? The surgeon who would adopt this mode of reasoning and act upon it, would in all probability precipitate the death of his patient, and forfeit his claim to the character of a sound practitioner.

There is another symptom to which the advocates of congestion attach great importance—as indicative of engorgement of the intestinal mucous membrane. I allude to the discharges of blood and bloody serum from the bowels, which form so alarming a feature in low fevers. If these discharges be the result of congestion, nature institutes a most effectual method to relieve it without the aid of medicine—a method which, from its violence, threatens to destroy the patient. It is not proved, however, that any remarkable injection of the mucous membranes precedes these discharges, nor is it necessary to suppose its existence. In the absence of positive proof upon the subject, two prominent causes may be suggested, furnishing a probable explanation of this alarming symptom. The first is found in the condition of the blood itself, and the second in the relaxed condition of the tissues through which it passes.

That the blood is a fluid possessed of a degree of vitality, is, I believe, generally admitted, while its vital and chemical condition is now acknowledged to constitute an important element in estimating the phenomena of disease. The revival of the humoral pathology under the new lights afforded by animal chemistry, must be regarded as an auspicious era in the science of medicine. Its complete overthrow, and the substitution of the exclusive doctrines of the solidists, by turning the investigations of pathologists into the circumscribed channel afforded by the latter, have probably retarded rather than promoted the progress of medicine. It is therefore gratifying to observe the tendency of modern inquiry; and to find some of the most eminent pathologists of our times directing their attention to this new field of research. The late work of Prof. Andral, on the physical alterations of the blood and animal fluids in disease, as well as the valuable memoirs of Laycock and of other British writers, furnishes ample evidence of the good results which are likely to flow from these investigations.

The old authors attributed much of the danger and violence of the class of fevers which they termed malignant, to a *putridity of the humours*; and although their theories based upon this idea, cannot be received as authority at the present day, they still point to a very essential element in the consideration of these diseases. That the blood is affected, in its vital and chemical properties, by epidemic influences in the atmosphere, cannot admit of doubt. This is seen most conclusively in the various modifications of the inflammatory process, which occur in particular seasons and localities. Let a patient receive a simple incised wound, for instance, during the prevalence

of an epidemic erysipelas; and instead of a healthy, adhesive inflammation or an exudation of plastic lymph, from the blood, whereby the cut surfaces will be united, a destructive process is instituted, which may spread to surrounding parts, and may even destroy life. Hence it has often happened in malignant fevers, that the incision made by a lancet in the act of venesection, has resulted in erysipelas and gangrene. The same thing occurs in malignant scarlatina—where mortification has been known to follow a leech bite, or has spread over and around a blistered surface—hastening the death of the patient. An alteration in the properties of the blood is further observed in those violent fevers in which the secretions are altered or arrested. Thus in the worst cases of malignant typhus, the blood passing through the liver does not give up its bile, or through the kidneys, its urine; when drawn from the arm it is black and tar-like, and fails to separate into serum and crassamentum. This has been called “dissolved blood,” and is seen entering the cellular tissue in the form of petechiae, issuing from the gums in drops, and running off from the bowels in large quantities.

The black vomit of yellow fever, and the bloody fluxes of the old writers, are of the same character. May not the discharges which occur in what is termed congestive fever, in its most aggravated form, result from a similar alteration in the blood? and, viewed in connection with the other symptoms, is it not much more rational to impute them to this source, than to suppose that they are the result of an irritation of the mucous follicles of the intestines, whereby a congestion of blood is invited to this surface? Did time permit, this branch of the argument might be extended, in order to show, that discharges of a similar character do occur under circumstances where no one supposes the existence of congestion. One case, which recently fell under my observation, I cannot avoid noticing as an illustration of this fact.

During the late riots, a man was brought into the Pennsylvania Hospital, mortally wounded by a ball passing through the abdomen, tearing up the walls of this cavity, and exposing the intestines, although without penetrating their cavity. Through the kindness of the attending surgeons, every facility was offered for examining the cases of these unfortunate patients—and this man attracted particular attention, from the fact of his throwing up from the stomach a dark, bloody fluid, resembling very much the black vomit of yellow fever, within an hour or two after admission. He was at this time cold and pulseless, affected with thirst and vomiting, and almost insensible. There was nothing in this case to invite congestion to the stomach—nor would his general condition have justified the supposition of its existence. The same thing not unfrequently occurs in the moribund state, or immediately preceding it, in many diseases—and is one evidence of the breaking up of the system.

In those cases of intermittent or remittent fever in which coma or delirium occurs, congestion of the brain is confidently stated to be present; and although this is a rare form of the disease, it is supposed to strengthen the general

conclusion as to its congestive character. Now it is well known that coma, delirium and convulsions occur in two opposite conditions of the brain—both from a redundancy and from a deficiency of blood in the organ. Examples of the former condition are numerous and familiar—while illustrations of the latter, although less generally recognized, are nevertheless frequently met with, and cannot fail to strike the unprejudiced observer.

The violent delirium of mania—the most intense mental excitement, is constantly met with in connection with a pallid and anaemic condition of the system, which can be traced to debilitating causes, and sometimes to the direct abstraction of large quantities of blood. Coma and convulsions often occur in hysterical females, or in married women after copious uterine hemorrhages. All the symptoms of oppressed brain, are seen in children wasted down by exhausting discharges from the bowels, or labouring under sympathetic affections of the organ dependent on intestinal irritation.

These, with a multitude of similar facts, should convince us of the absurdity of pronouncing upon congestion of the brain, from the fact that there is coma, delirium or convulsions in a given case, without taking into consideration the state of the general system, and the various attending circumstances.

I have now presented an analysis of some of the symptoms which distinguish what is termed congestive fever, and which are supposed, in an especial manner, to indicate its congestive character. It has been my object thus far to prove from negative rather than from positive evidence, that this view of the subject is erroneous, and to point out, by reason and analogy, that all these phenomena may and do occur under circumstances which forbid the idea of congestion, that the various organs which are disturbed in the progress of this fever, are similarly affected by causes of a depressing character acting upon the nervous system; that nervous shocks from accidents, operations, &c., will produce them; that they may even arise from hemorrhages when the system is drained of blood; and that the discharges which sometimes mark this disease, may be fairly attributed to alterations in the blood itself, combined with laxity of fibre.

It has been before remarked, that no *positive* evidence of the existence of congestion has been advanced by the advocates of that theory—their whole argument being based upon the language of the symptoms, which conveys to one mind a meaning the reverse of that which is comprehended by another. Post-mortem appearances might furnish data for more positive conclusions, although even these may be subject to ambiguity when viewed independently of other circumstances. It is somewhat remarkable that in all the accounts of a disease so confidently styled congestive, no post-mortem appearances are recorded; nor does it appear that examinations have been made, at least by our American writers. This is probably attributable to the season of the year in which the disease occurs, to the tendency of the bodies to rapid decomposition, and to the incessant occupation of the physicians during the

epidemics, leaving them no time for prosecuting such investigations. So far, however, as pathological anatomy has illustrated the pathology of fevers generally, no confirmation is given to the doctrine of congestion.

The interesting researches of Dr. Stewardson, in reference to remittent fever, of Drs. Gerhard and Pennock in regard to typhoid and typhus, and those of Louis on the yellow fever of Gibraltar, manifest particular changes in the organs, not sufficient to account for all the symptoms of these several varieties of fever, and deemed by them as secondary to a pre-existing condition of some other portion of the body.

In reference to remittent, which is of malarial origin, and intimately related, if not identical with what is termed congestive fever, the latter being a high grade of the former, Dr. Stewardson remarks, "Independently of the spleen, the liver was the only organ uniformly altered in the cases which we have examined, but as this alteration was certainly not inflammatory, it is impossible to regard the fever as symptomatic of such a lesion. We are thus driven to seek the essential element of the malady in some other portion of the economy. That the blood is very generally, if not always, diseased, is highly probable, and although the opinion which would attribute remittent fever to this cause, be incapable of rigorous proof, it seems to me, that it possesses very much the same degree of probability as a like opinion in reference to typhoid and typhus fevers."*

In the dissections instituted by Drs. Gerhard and Pennock, at the Philadelphia Hospital, during the epidemic of typhus fever which occurred in 1836, no invariable characteristic lesion of the tissues or solids of the body was discovered, but the tissues were *permeated with a dark, dissolved blood*; in some instances the spleen was softened, as well as the greater tuberosity of the stomach.† These references to the late developments of pathological anatomy in regard to other fevers, are not, of course, assumed as applicable to those termed congestive, as they have not been the subject of special examination; they are merely noticed as confirmatory of some of the positions taken in this paper, and as evincing the fact, that so far as post-mortem investigations have proceeded, under the guidance of the most acute and rigid observers, they have not discovered congestion as a cause of death, or as an essential element in fevers of a low grade, having many points of resemblance to those under review.

There is one other species of evidence, which I shall adduce of the non-congestive character of this fever, which partakes somewhat of a positive character, and to which I invite special attention. It is the effect produced by therapeutic agents in controlling and arresting the violent symptoms.

It has been before remarked, that consistency would require an adoption of the depletory practice, on the part of those who believe congestion to be the primary and most alarming pathological condition in this complaint. If an

* See Stewardson on Remittent Fever, *Amer. Journ. Med. Sci.*, April, 1842, p. 292.

† See Gerhard on Typhus Fever, *Amer. Journ. Med. Sciences*, Feb. and Aug. 1837.

organ is oppressed by an accumulation of blood in its structure, distended beyond its natural dimensions, and its functions thus suspended by mechanical obstacles, the most rational course would seem to be, to abstract a portion of the superabundant fluid, in order to enable the system to rise above the weight of disease which oppresses it.

It is upon this principle that general and topical depletion is recommended by many high authorities in the treatment of this as well as of other forms of malignant fever; and if the theory of congestion were true, it should be the only successful method of practice. But what does experience teach in reference to this matter?

The most intelligent and experienced physicians in those districts in which this fever prevails, (if our information is correct,) condemn the depletory practice as dangerous, and attribute much of the frightful mortality which has prevailed at particular periods, to its indiscriminate adoption.

Some of these practice topical depletion to a limited extent, to relieve, as they say, local congestions, in conjunction with stimulants, tonics and re-vulsives. This is the practice recommended by Dr. Parry. Dr. Wharton considers capsicum in large doses almost a specific for the intense thirst which prevails. The vomiting, dyspnea and oppression are, he states, relieved by stimulants—amongst which he ranks brandy as the best. He has seen the most intense headache and restlessness instantly relieved by a large dose of this article.

An intelligent practitioner of Tennessee has informed me that he believes many of the ordinary intermittent and remittent fevers in his district might be thrown into the dangerous form called congestive, by copious depletion, or a large dose of tartar emetic; and there is no doubt many simple cases become alarming and fatal by injudicious practice.

After the establishment of reaction following the chill or cold stage, both Drs. Parry and Wharton rely solely upon large and repeated doses of quinine, to prevent the accession of another paroxysm. By some recent authorities, quinine is given through all the stages both in remittent and intermittent, and it is said, with the happiest results. The testimony in favour of this practice, from various quarters of the country during the past year, is very strong, and calculated to break in upon the long-established prejudices against the use of bark or quinine in the early stages of remittent fever. Dr. Wharton says of it, "In this town, (Grand Gulf, Miss.,) quinine was scarcely used, till the latter part of the summer of 1837. The great mortality which prevailed then, when contrasted with the present success in this disease, is to us, who have witnessed it, one of the strongest proofs which could be adduced of its inestimable value." And Dr. Parry says, "Although I like all the collateral assistance I can receive from other articles, yet the sulph. quinine is the remedy. It is the master article of the *materia medica*—with it, and reaction once established, I believe nearly every case can be cured, and without it, scarcely any recover."

If quinine be a remedy so potent as to disarm this frightful malady of its

terrors; and if brandy and other diffusible stimuli are proved to be serviceable in relieving many of the symptoms which threaten the speedy destruction of the patient—then the theory which attributes these symptoms to congestion, must either be abandoned, or the ground must be assumed, that congestion of the vital organs is relieved by the two opposite methods of depletion and stimulation. Rather than to occupy this contradictory position, would it not be wise to abandon the term *congestive*, as vague and delusive, and seek for one more in accordance with the views which experience sanctions and reason approves?

The word *malignant*, which denotes a fever produced from an intense contagion, or from a malarial poison, attended with violent and dangerous or mortal symptoms; or its modern synonyme, *pernicious*, would perhaps be sufficiently intelligible to most minds; and would, at least, be free from the objections here urged against that now in use. A more definite and descriptive term, however, is, I think, to be found in the word *adynamic*, from the Greek root, signifying want of power—thus designating the primary element and most conspicuous feature in the disease. This term has been adopted by some of the late British writers, to distinguish the class of fevers formerly known as *typhus*—and as the great object of words is to convey a distinct and uniform meaning, free from ambiguity, and not liable to variety of construction, it appears to possess an advantage over both *malignant* and *typhus*—the peculiar cause and type of the malarious fevers giving them a distinctive character, in connection with the nervous prostration which attend them. I can see no objection to this nomenclature, even on the part of those who adhere to the idea of passive congestion, dependent on a want of power in the heart and blood-vessels, and secondary to the depressing influence of the malarial poison on the nervous centres; whereas, by retaining the term *congestive*, they are designating the disease by one of its less important symptoms, and giving the sanction of a name to a practice which they deprecate.

Similar objections to those here stated, might be urged against the use of the word *inflammation*—as applied to the explanation of a great variety of diseased phenomena, having no special relation to each other, and requiring opposite plans of treatment. In fact, in the estimation of many physicians, *inflammation* and *congestion* form the basis of the whole fabric of pathology. They can conceive of no important disease without the existence of one of these two states; and they attribute all the lesions which are observed after death, to the previous occurrence of one or the other of these processes. It is to be hoped, however, that a new era is dawning upon medical science; and that with the increasing light which we are constantly receiving upon many abstruse physiological questions, and more especially upon the composition and vital properties of the fluids, that more comprehensive views of the nature and treatment of disease will be generally diffused, and that medical nomenclature will be gradually adapted to the new facts and principles which the researches of science may unfold.